Reviewer’s report

Title: The development of socioeconomic health differences in childhood: results of the Dutch longitudinal PIAMA birth cohort.

Version: 1 Date: 13 September 2010

Reviewer: Gwenn Menvielle

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This paper deals with socioeconomic inequalities in childhood and how they develop from age 0 until age 8. The analysis is based on a large cohort.

I have several major remarks, all are major compulsory revisions.

1. Important aspects are missing in the method section.

The authors state that « determinants are considered explanatory when education health disparities are reduced by 15% or more ». There is no justification for the choice of this cut-off point.

More detailed information should be given about the method used to create the imputed dataset. Many readers are not familiar with this method. Which predictors have been used? Also, in the text, the authors mention « five imputed datasets » and then state that they « report results from the analyses of the imputed dataset » (dataset is singular, suggesting that they used only one dataset). This should be clarified.

2. The tables should be revised. They should be self-explanatory. Please add footnotes to the tables.

Table 4. The confidence intervals should be added.

3. The discussion part is poor.

The authors used maternal education as an indicator of the SES. This choice and its possible implications on the results are not discussed. What if they had chosen a SES measure related to the father, or to the family? When we study health in childhood, the characteristics of the family are relevant, in particular whether the mother is a lone mother. The financial aspect is also relevant. All these issues are not present in the manuscript and should be discussed. Do the authors have information on these aspects in their cohort? If so, did they perform sensitivity analyses? For instance, if the results substantitally change when we account for lone mother, then the conclusions also differ in terms of public health recommendations.

4. The possible explanation for health differences should be more discussed. They used day-care attendance in the first year. Why did they not consider day-care attendance before the child goes to school?

5. The explanatory factors are measured at birth or around birth. When we study health until the age of 8, other factors, measured later in the child’s life, could
play a role. For instance, the authors do not mention diet or physical activity, which are likely to play a role among children. This may be particularly relevant for overweight and obesity.

6. The measure of smoking is relatively crude. For the mother, it is smoking during the first four weeks of the pregnancy, so basically before the woman knows she is pregnant or until she realises she is pregnant. In the discussion, they mention the effect of maternal smoking during pregnancy on the child’s health. Do the authors really consider the variable they use dis a proxy of maternal smoke during the pregnancy. The other measure of smoking is more than once a week at home. Why did they author chose this definition for environmental tobacco smoke? A more expanded discussion on the tobacco variables is needed.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests